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Date: Sat, 9 Jul 1988 22:09:41 EDT

From:

Subject: Miscellaneous re history of bacterial genetics

To: Joshua Lederberg <JSL@ROCKVAX>

I just finished your fine piece in Ann. Rev. Genet. on bacterial recombination. It gives me a large number of interesting leads to follow. Many thanks. There are three small matters that stimulated immediate reactions. (I haven't time for more at the moment; I'm leaving for a week tomorrow.)

1. Re Boivin's loss of replicability of transformation in E. coli. Slonimski was a member of a class of some 6 - 8 grad student/post-docs who took a course on DNA techniques with Boivin. Even the incompetent were able to get transformation. The system was lost shortly after. Boivin either was moving to or had just moved to Strasbourg. Slonimski's speculation is that it was not a strain problem, but a water problem -- too much or too little calcium in the water in the new location, or some such. The stable replicability had lasted some 3 - 4 years before the system was lost. All this should be rechecked; I'm reporting from memory S's comments after a talk that I gave. (My secretary will send out a huge raft of my stuff to you Monday; that talk should be included. It's called "Disciplinary Specialization and the American 'Dis-solution' of the Impasse Between Heredity and Development" or some such. (It's one of three variant versions of a talk on the theme of the conflicts between embryologists and geneticists over the fundamentality of Mendelian inheritance.) Please throw out the things that look like they won't interest you; the package is inordinantly large. It DOES include an old flyer on the Oxford series -- a direction in which I hope you will take your eventual book.)

2. I'm very curious about your reading in that summer of 1947 at Woods Hole. Do you have notes from that summer? Can you recall the configuration that the history of microbiol. took on for you on the basis of that reading? What place did you think that genetics would have in microbiology at that time? Can you reconstruct what you read -- or the central documents that stuck out at the time? For someone pursuing the history, it would be of great value to know which documents seemed important or on a sound track and which seemed silly and old fashioned or marred by off-the-wall speculations as of that particular moment.

3. It is not my focus, but the things you say on p. 39 regarding 'allusions about character and race' clearly deserve further pursuit. For instance, was the Rockefeller Foundation ready to face down those institutions that discriminated or did it, too, share some of the same biases? How is it that such midwestern enclaves as Wisconsin, Illinois, and Indiana came, at that time, to break down some of the barriers with relative ease -- or is that a correct description? How did Wisconsin arrive at the idea of a microbial geneticist? Who in that relatively conservative seeming crowd (from this distance, with no direct knowledge of the people and considerable ignorance of who actually was there) would have steered things in such a direction? Is Ray Owen's review available? of any value? Is all of this a good marker of a conceptual or sociological shift, marking the new entry of a postwar generation into the biological mainstream rather more quickly than I would, a priori, have expected? Most especially, for my own purposes, does it mark a change in thinking about the content or character of genetics or a shift in the content of genetic theory? There's a lot

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to be teased out here I think.

Thanks for letting me try to pique your interest. If you have time and find these questions interesting, I'd love to have your response. But don't waste time on this stuff if (as I expect) you're too busy. I will not be online for a week in any case.